

lished dealing with the lighter aspects of Indian life. It is pleasant to note that Herr Schmidt found the English officials and planters everywhere very hospitable and cordial, ready to assist him in his inquiries as to the people, and able to give him much valuable information on the subjects which he was studying.

By Vocal Woods and Waters. By Edward Step. Pp. 254. (London: Bliss, Sands, and Foster, 1894.)

A RESURRECTION book, made up of papers originally contributed to *Good Words*, *Leisure Hour*, *Sunday Magazine*, *Silver Link*, and other periodicals. The author is well known as a close observer of nature, and he has the amount of poetry in his composition essential in a writer on popular natural history. The book is nicely printed and illustrated.

LETTERS TO THE EDITOR.

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Acquired Characters.

IN reference to the question as to how far the signification of the term "acquired characters" may usefully be extended beyond the precise limit given in Lamarck's two laws (quoted in my letter of November 15), or what phenomena may be brought into close relationship with those indicated by Lamarck, the following considerations, will, I think, be found useful.

Let us consider a relatively stable local "race" of some species of organism. The race is found to present a certain range of variation, but has an average character; and cases approximating to the average are so far more numerous than departures from it, that for our immediate purpose we may leave the aberrant individuals out of account. The average specific character is a matter which may be determined by measurement and weighing. It can be stated in numerical form as to length of such and such parts, breadth and depth of other parts, weight (*i.e.* amount) of pigment or other chemical product here or there. We know by experiment that these quantities can be altered in immature individuals *within limits* by changing the physical conditions in which the individual is placed. These physical conditions are (roughly speaking) such measurable quantities as those relating to temperature, light, mechanical strains, moisture, and varying amounts of chemical compounds or elements operating on the organism through its absorbing surfaces (respiratory organs, digestive organs, integument).

From such experiments we are led to conclude that without destroying the life of an individual many characters can be increased in quantity, but that there is a limit beyond which they cannot be so increased; and that many characters can be reduced in quantity in the same way in an individual without destroying its life, but that here also there is a limit. Hence it seems that we are justified in distinguishing the "potential" from the "actual" characters of an organism.

This *potentiality* of the individual is something inborn or congenital. On the other hand, the *actual* quantitative condition of the average characters of a naturally-occurring assemblage of individuals is necessarily to some extent the result of the operation of the measurable physical agencies which constitute the normal environment of the species or race. (The range of difference, it may here be noted, between the potential and the actual characters of a species as thus indicated, is found to differ very greatly in different species and in regard to different parts of the organism.) Thus then every individual exhibits certain quantitative characters, the amounts of which are *determined* by the operation on the individual of given and related quantities of external agencies. To a character thus quantitatively determined, some writers have extended the term "acquired character," inasmuch as it is not the congenital potential character in its purity (if such a thing were possible) which we thus contemplate, but the congenital character as moulded, increased, or restrained by surrounding conditions. But whilst I am very decidedly of opinion that a consideration of this moulding, expanding, or restraining influence of the normal environment is likely to throw

important light upon the implications of Lamarck's doctrine, I agree most emphatically with Mr. Francis Galton in thinking that the use of the term "acquired characters" must be limited, as indicated by Lamarck's own statement, to characters, "which are regularly found in those individuals only which have been subjected to certain special and abnormal conditions" (to quote Mr. Galton's words). The word "acquired" was used by Lamarck, and should continue to be used as pointing to an acquisition, under *new* conditions, of *new* character or characters, distinct from the normal characters which form, as it were, the starting-point, however determined or brought into existence.

It is, however, true that the difference between the actual characters of an individual organism as compared with its potential characters—a difference the origin of which may be expressed by calling the former "responsive characters"—is of the same order whether the actual characters are determined in their amount by the normal environment of the race, or by abnormal quantitative changes of that environment.

And it seems to me, that in considering this we are led to the conclusion that the second law of Lamarck is a contradiction of the first. Normal conditions of environment have for many thousands of generations moulded the individuals of a given species of organism, and determined as each individual developed and grew "responsive" quantities in its parts (characters); yet, as Lamarck tells us, and as we know, there is in every individual born a potentiality which has *not* been extinguished. Change the normal conditions of the species in the case of a young individual taken to-day from the site where for thousands of generations its ancestors have responded in a perfectly defined way to the normal and defined conditions of environment; reduce the daily or the seasonal amount of solar radiation to which the individual is exposed; or remove the aqueous vapour from the atmosphere; or alter the chemical composition of the pabulum accessible; or force the individual to previously unaccustomed muscular effort or to new pressures and strains; and (as Lamarck bids us observe), in spite of all the long-continued response to the earlier normal specific conditions, the innate congenital potentiality shows itself. The individual under the new quantities of enviroing agencies, shows *new* responsive quantities in those parts of its structure concerned, new or "acquired" characters.

So far so good. What Lamarck next asks us to accept, as his "second law," seems not only to lack the support of experimental proof, but to be inconsistent with what has just preceded it. The new character, which is *ex hypothesi*, as was the old character (length, breadth, weight of a part) which it has replaced—a response to environment, a particular moulding or manipulation by incident forces of the potential congenital quality of the race—is, according to Lamarck, all of a sudden raised to extraordinary powers. The new or freshly-acquired character is declared by Lamarck and his adherents to be capable of transmission by generation; that is to say, it alters the potential character of the species. It is no longer a merely responsive or reactive character, determined quantitatively by quantitative conditions of the environment, but becomes fixed and incorporated in the potential of the race, so as to persist when other quantitative external conditions are substituted for those which originally determined it. In opposition to Lamarck, one must urge, in the first place, that this thing has never been shown experimentally to occur; and in the second place, there is no ground for holding its occurrence to be probable, but, on the contrary, strong reason for holding it to be improbable. Since the old character (length, breadth, weight) had not become fixed and congenital after many thousands of successive generations of individuals had developed it in response to environment, but gave place to a new character when new conditions operated on an individual (Lamarck's first law), why should we suppose that the new character is likely to become fixed after a much shorter time of responsive existence, or to escape the operation of the first law? Clearly there is no reason (so far as Lamarck's statement goes) for any such supposition, and the two so-called laws of Lamarck are at variance with one another. To push the matter further—in those cases in which experiment has been made, it has been found that a character acquired by an individual removed from the operation of a related condition normal to the race of which that individual is an example, is replaced by the old character when the old condition is restored in the environment of the offspring of that individual. No doubt I shall be challenged to

produce evidence confirming this last statement. I admit that carefully studied and conclusive instances are not very numerous, but I refer to such cases as the non-transmission (a) of plus or minus variation in pigment produced in individuals by greater or less exposure to sun-light; (b) the effects of dry or moist climate on individual plants; (c) the effects of change of diet on individual animals; (d) the effects of increased use of muscles in men and animals.

It seems that we are driven to the conclusion that the causes which have been active in producing changes the accumulation of which amounts to specific, generic and larger differences, must be causes which are able to act upon the potential congenital quality of the individual, and that there is no reason for associating the somewhat superficial and late responses or reactions of the parts of a growing individual to normal or abnormal forces of its environment with that more subtle and profound disturbance, which is permanent and affects the potential character of the germ, and more or less of all the germs derived from it.

At any rate this is the absolutely unanimous testimony of all those observers, in all countries and in all ages, who have been practically concerned (often with vast pecuniary interests at stake) in the production of relatively permanent new races of animals and plants. Breeders of horses, cattle, sheep and dogs, pigeon and poultry-fanciers, crop-growers, nurserymen, tulipomania, and the like, have never in any single instance put the Lamarckian principle into practice. On the contrary, they laugh it to scorn. Not one of them ever produced a new race by moulding the parents. But, on the other hand, they do subject the selected parents to novel and disturbing conditions, to which the changed characters of the offspring (not of the parents) have no "responsive" relation; they cross-breed here and cross-breed there, until the "specific potential" is broken-down, and strange unlooked for varieties are born and grow up irrespective of normal or abnormal environment. From these congenital variations they select desired forms, and perpetuate them with perfect assurance and security.

For the present I see no evidence of a production of new races on the face of the earth, excepting by the method adopted by these men, viz. by the selection of congenital variations; such congenital variations being produced as the result of (but without any direct adaptational relation to) a disturbance of the material of the reproductive particles of both sexes; that disturbance being increased, if not determined, by changed environment of the parental organisms or the coupling of remote strains.

E. RAY LANKESTER.

Oxford, November 17.

The Present State of Physiological Research.

THE extracts given in NATURE of November 15, from an article by Prof. Max Verworn, of Jena, on "Modern Physiology," will serve to draw the attention of biologists to the reawakening of interest which is now evinced by many physiologists in regard to the fundamental phenomena exhibited by living things. As the opinion of physiologists is expressly invited in reference to the questions raised in this article, I venture to express my own as being in the main the same as that of Prof. Verworn.

It seems to me an obvious truism to say that the methods which can ensure a real advance in general biological knowledge must be those in which comparative physiology takes the lead. In my recent presidential address to the Liverpool Biological Society I urged the establishment of laboratories for the systematic study of the comparative physiology of the simpler organisms, the end in view not being the elucidation of the functions of organs with an *arrière pensée* as to their relation to man, but the examination of the activities for their own sake, since this inquiry forms the only means of approaching the mystery which enshrouds the essence of living existence.

The determination of the reactions of simple organisms to physical changes (stimuli), and the grouping of such resultant effects carried out systematically, form a line of physiological inquiry of transcendent importance, both because of its large scope and fundamental character, and because it opens the way towards the partial elucidation of the physiologist's real problem. This problem is the one involved in the question, to what extent all living phenomena are to be regarded as reaction phenomena? Are we, on the other hand, compelled to postulate the existence in every living thing of a *deus ex machina*

which can, if it will, act independently of every physical stimulus, a so-called "vital force"?

Prof. Verworn is right in forcing upon the attention of physiologists the paramount necessity for work of this kind. I venture, however, to point out that he has not done justice to the judgment of his contemporary physiologists if, as I imagine, he has been led to infer from the character of the mass of current physiological work, that they do not realise the importance of such comparative physiology. But to realise the importance of an inquiry and to be able to carry that inquiry into effect, are unfortunately by no means identical positions.

There are undoubtedly obstacles to the latter, however ardently we may desire its fulfilment.

In the first place physiology is, in this country, more or less shackled by its position. It owes everything to medicine. Its laboratories are adjuncts to medical schools, its professors must take their share in the teaching in such schools, and this teaching is essentially connected with human physiology.

The debt, which as physiologists we owe to medicine, is one which we gratefully acknowledge, but even with the thanks on our lips we may be supremely conscious of the chains which still hang on and impede the debtor. It is this close relationship which, in my opinion, has served to accentuate the separation between physiology and the science of which it properly forms part, biology; a separation which is now almost a judicial one, and if unchecked may become an actual divorce. It is rare to find a physiologist who has been highly trained in zoological investigation, and rarer still to find a zoologist who has attempted to perfect himself in the methods used in physiological laboratories. Yet the appropriate blend is essential for the advent of those comparative physiologists who alone can do full justice to the systematic inquiry now advocated.

Another difficulty in this country is undoubtedly due to the scanty pecuniary help afforded to scientific work which is neither technical nor directly concerned with what is regarded as the public good. Physiology, to-day, is maintained in Great Britain solely because it forms an essential part of a specialised technical education, that of the medical student; it is not maintained in order to inquire into the mystery of living things as such.

In order to adequately develop such an inquiry as this, it would be necessary to have a new department furnished with the equipment of both a zoological and a physiological laboratory, and with skilled workers who have leisure to prosecute their investigations. Since this means money, its full establishment may have to be postponed until that pious benefactor appears whose dawn even a Bodleian librarian has now anxiously to await.

Finally, I do not think the outlook is so discouraging as Prof. Verworn seems to believe, nor that "we are making no progress in physiology."

He admits that during the last twenty years we have attained to a precision in our experimental methods such as excites the astonishment of the uninitiated; and surely the mastery of method is the first step, and that an invaluable one, towards its future more fruitful employment. I do not imagine that even the systematic physiological investigation which he advocates, will involve the employment of new methods to the exclusion of old ones; it is the material which will be novel, not the entire experimental technique. Isolated instances of the application to simple excitable organisms, of such physiological methods as have been employed in elaborate detail for the investigation of muscle, nerve, &c., are well known to us all, and to no one better than Prof. Verworn; the real desideratum is surely that the instances should be no longer isolated, but form part of a broad systematic inquiry.

FRANCIS GOTCH.

University College, Liverpool,
November 17.

Wilde's Theory of the Secular Variation of Terrestrial Magnetism.

MR. WILDE'S reply in NATURE of October 11 to my letter of criticism in the same of August 9, with respect to his communication to the Royal Society, contained in the *Proceedings* for March 1894, has just come to my attention.

As the letter consists entirely in an attempt to show the inaccuracy and unreliability of my statements with respect to the inclination-observations made at St. Helena,